ing problem and devises a method of solving it, the catastrophes that underlie the method's implementation have been left far behind. For example, a digital computer is full of catastrophes—one occurs every time a flip flops—but they are irrelevant to the description of what a program does and how. Catastrophe theory characterizes the passage from the continuous to the discrete, but it has nothing to say about complex descriptions or procedural specifications that are written and executed in a language whose entities are already discrete.

2) *Automata theory.* This and the following approaches rest on less sophisticated mathematics. The argument is rarely formulated, but proceeds roughly as follows. The brain is made of locally active pieces, glued together in an essentially simple way. The way to understand it is therefore to study the class of computations that can be performed by an abstract entity that captures certain properties of local autonomy and simplicity of interconnection.

Such devices are called cellular automata, and Merzenich's paper provides a readable introduction to them. They are of interest in their own right, but studying them will not provide insights into the workings of the brain. The reason is that very weak machinery produces a computational engine that is universal; and given a particular method and moderate ingenuity, one can usually devise an automaton that implements it efficiently. The important question is, What processes need implementing? Abstract studies cannot help to answer this, because they introduce no notion of what constitutes a *useful* process. To do this, you have to study information-processing problems, not particular pieces of computing machinery.

3) *Learning automata theory.* This theory says that since learning occurs in the brain we shall come to understand it by studying automata that change themselves (sometimes called "self-organizing systems"). The papers by Dal Cin and by Vollmar are examples from this volume describing work that appears to be founded on this view. The theory deals only with changes that occur at a very low level, yet except in simple negative feedback situations it is not at such a low level that the interesting phenomena of learning are captured. Anyone who has ever written a machine-code program knows that random low-level changes in the definition of a procedure cause havoc. And again, digital computers have always had "memories," yet psychologists do not find the kind of "learning" they do very interesting. The point is that although at some stage a low-level change must occur, why the change does what it does requires an explanation at a much higher level of description. (There is an analogous argument about genetic programs, which says that most of the time evolution cannot proceed by changing random instructions in the machine code; the lowest level at which viable changes can be made is that at which subroutine calls can be altered.)

4) *Neural net theory.* This combines the limitations of the two previous theories, and arises from a belief that there is something computationally very special about neurons. This belief lies behind much of the section on network physiology in this book. Experimental biologists as well as theoreticians are prone to this error, but its effects for them are less disastrous. If one studies the details of synaptic transmission out of a belief that it will throw light on the computations performed by the brain, it is not fatal that the belief is mistaken, because something interesting about synaptic transmission will probably emerge.

The neural net theory states that the brain is made up of neurons, connected either specifically (for small structures) or randomly (for large ones). Hence, in order to understand the brain we need to understand the behavior of these assemblies of neurons. Here there are two problems. First, the brain is large, but it is certainly not wired up randomly. The more we learn about it, the more specific the details of its construction appear to be. Hoping that random neural net studies will elucidate the operation of the brain is therefore like waiting for the monkey to type Hamlet. Second, given a specific function of inevitable importance (like a hash-coded associative memory), it is not too difficult to design a neural network that implements it with tolerable efficiency. Again, the primary unresolved issue is what functions you want implemented, and why. In the absence of this knowledge, a neural net theory, unless it is closely tied to the known anatomy and physiology of some part of the brain and makes some unexpected and testable predictions, is of no value.

5) *Characterizing the computational power of a system.* Another way of approaching biological information processing is to attempt to prove that a system—a set of enzymatic pathways, for example—is in principle as computationally powerful as some class of finite automaton (as is done in Rossler's paper and in others in the section on molecular and modifiable automata). This is interesting, but probably a waste of time. On the other hand it is not a waste of time to take a specific structure, such as an oak leaf or a chick wing, and ask what process could generate one, subject to the constraint that if changed slightly the same process could be used for making, say, an elm leaf or a chick foot.

The mysteries of development and of the central nervous system will ultimately be explained in terms of processes, data structures, virtual machines, methods, algorithms and the particularities of their implementation, control structures, and types and styles of representation of knowledge together with detailed specifications of the knowledge required for different tasks. A novel feature of the contemporary scientific scene is that the computer allows one to try out information-processing theories on real-world data. One can argue that a clever enough scientist might not need direct computational experience to formulate the appropriate methods and prove that they will work; but the intuitions needed for understanding biological information processing are not easily available. Only by wresting them from actual experience does one gain a feel for what questions need to be asked, and develop a language in which to ask them. Even with this help, progress is slow, and only small advances have so far been made. But without it, larger ones never will be.

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Periodicity

Circannual Clocks. Annual Biological Rhythms. Proceedings of an AAAS symposium, San Francisco, Feb. 1974. ERIC T. PENGELLEY, Ed. Academic Press, New York, 1974. xiv, 524 pp., illus. $22.50.

It comes as no surprise to any of us that biological events are timed precisely and periodically on a yearly basis corresponding to the earth's travels about the sun. Truly remarkable, however, is the fact that when organisms are isolated experimentally from the obvious cues of the yearly cycle the events may still be precisely and periodically timed. In this situation, the period of the cycle is not exactly one year, but deviates from it. E. T. Pengelley initiated the use of the word "circannual" to refer to these persistent rhythms, and the word carries the implication that the rhythms can be generated from within the organism.

Circannual Clocks is a collection of papers from a symposium. The volume will not be rapidly outdated because, as Menaker notes in his concluding remarks, "the major difficulty in the study of circannual rhythms is a consequence of the ratio of the period length of a single circannual cycle to the length of the productive life of the biologist!" The volume begins with a dedication to William Rowan in honor of his classic work on the junco in which he showed increases in the duration of illumination to be associated with developmental changes in the testes. A short biography by Rowan's research assistant, Robert Lister, followed by a reproduction of Rowan's 1925 *Nature* paper introduces the volume.

One central concern of the papers in the book is the demonstration of persistence of the rhythms in constant conditions, that is, a cataloging and documentation of the existence of circannual rhythms. The most convincing data are those for hibernation in ground squirrels (data from Heller, Poulson, Pengelley, Asmundson). The recognition of hibernation as based on an endogenous rhythm by Pengelley and Fisher (1957) generated interest in the field, and it is appropriate that the volume is edited by Pengelley. Other physiological variables in a variety of species are suggested as having circannual rhythms: body weight, locomotor activity, food and water consumption, and reproductive competence in ground squirrels; aging, growth, and development in a marine colonial cnidarian; reproduction in cave crayfish; molt, body weight, nocturnal restlessness, testis size, juvenile development, and orientation and migration in birds; and antler replacement in deer.

The field has progressed beyond the demonstration of the existence of circannual rhythms, and some of the new directions are delineated in the book. While the circannual clock has apparent endogenous components, it is clear to all the investigators that in the normal environment the clock is synchronized with (or "set" by) environmental cues so that the biological functions occur in their turn at the appropriate seasons. Light may be one such important cue, or "zeitgeber." Other possible time cues such as temperature changes, food availability, and prior physiological state are considered.

Problems associated with the circannual clock besides its zeitgebers are covered. The effect of temperature on the working of the clock has been studied, and the clock appears to be temperature-compensated. The possibility of a blood-borne trigger for the endogenous cycle is being explored. The conditions that will produce a persistent circannual rhythm in the laboratory may depend on the period of the constant light-dark cycle maintained. Furthermore, the period of the circannual rhythm can be forced to synchronize with artificial "annual" photoperiods—Gross found that the cycle of antler growth in deer could synchronize with cycles of from 2 to 24 months.

Some of the aspects of the circannual rhythms (persistence, zeitgebers, temperature compensation) are of concern also in studies of circadian rhythms. In the summary paper, Menaker considers whether exploring the analogy between circadian and circannual rhythms is likely to be fruitful for further studies and concludes that the most productive approach will be to study the "series of sequential steps of which circannual rhythms are surely composed" at the physiological level and that, because of the length of the circannual rhythm, some studies analogous to circadian studies are impractical.

A reading of this book leaves one convinced of the existence and interesting nature of circannual rhythms and curious about their physiological origins. The evolutionary justification for circannual clocks is considered and remains unexplained. While it is clear that a "calendar" would be of use in anticipating and adapting to seasonal events, it is not clear why an endogenous calendar would be necessary when there are so many environmental cues. This is just one of the many thought-provoking questions raised in the book.

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Life on a New Island

Surtsey. STURLA FRIDRIKSSON. Halsted (Wiley), New York, 1975. x, 198 pp. + plates. $14.95.

In November 1963 an eruption took place on the Mid-Atlantic Ridge 30 kilometers south of Iceland and a new island, Surtsey, was formed. This book is the story of the island, and of the living things that reached it, in the first 10 years. It is written in easy style and is not intended as a formal evaluation of research, but it is full of interesting observations and is well documented.

The Surtsey Research Association was organized within months of the first eruption, and coordinates the work of many observers and scientists. It is apparent that the Surtsey event is providing an opportunity to study dispersal movements and processes of colonization under boreal conditions over the range of a whole fauna and flora. It is apparent also that it is giving a valuable impetus to ecological work in Iceland generally.

Surtsey began as a single cone of tephra and ash, and a second vent was added later. Two smaller island cones formed nearby, but were quickly eroded. As the main island grew, the sea was walled off from the pool of magma and the explosive phases of the eruption came to an end; a denser lava then flowed out and covered much of the surface, and Surtsey was assured of a certain permanence. It is 2.5 square kilometers in area and 172 meters in height.

Sea birds landed as soon as the surface was cool, and various species are now nesting; they depend on the ocean for food and their excreta and carcases are the only considerable source of organic material inland. Marine life and flotsam thrown up by the waves provide the impermanent beginnings of soil in a narrow zone on the upper part of the beach, and this has now been colonized by a few halophytes such as sea rocket and lyme grass and a still smaller number of scavenger insects. But the porous ash and lava retain no moisture, and further progress is difficult. The ash is still devoid of macroscopic life, but in 1967 mosses appeared on the lava and have increased steadily in range and variety. These mosses, with the lichens and smaller organisms that shelter among them, seem to represent the first step toward the grassy heathland of adjacent islands.

But in sharp contrast to the slow development of the initial biota, there is a remarkable inflow of viable organisms by air and on the surface of the sea. "Showers" of spores, seeds or other parts of some 40 species of vascular plants, 158 species of insects, and so on, have been observed in the short space of 10 years. Reflecting on these observations, Fridriksson suggests that the fauna and flora of Iceland itself may well have been built up, species by species, within the postglacial period; there is no need to propose, as many authors have done in the past, a more ancient origin by land bridges from Europe.

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Materials Science

Diffusion in Solids. Recent Developments. A. S. NOWICK and J. J. BURTON, Eds. Academic Press, New York, 1975. xiv, 492 pp., illus. $45. Materials Science and Technology.

The study of diffusion in solids has had a long and exciting history. Research in the field has not merely provided material that may be of routine interest in relation to reaction rates in solids but has also yielded special insights concerning the ways in which normal and defect structures combine to produce effects that are often unex-